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[illegible]

Itinerary:

June 1[illegible]-June 15: Traveling to Los Angeles by train.

June 16- Los Angeles. Visited North American Aircraft.

June 17-June 19: La Jolla. Working at Scripps Institution of Oceanography with Munk, Sverdrup, and Arthur. Also visited [illegible] Naval Electronics Laboratory in San Diego.

June 20-June 21- Pasadena. Visited [illegible] and [illegible] at Cal. Tech. and talked with [illegible] and [illegible]blust.

June 22- Traveling from Los Angeles to Berkeley.

June 23- Spent most of the day with Johnson and O'Brien at Univ. of Cal. Engineering Div.

June 24-June 28- Took a vacation in the [illegible].

June 29-30- Stayed at Palo Alto and talked with various people at Stanford, including [illegible], [illegible], [illegible], and [illegible].

June 30-July 1- Returned to New York by plane.

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Thursday, June 12-Sunday, June 15. Traveling by train. Wrote a paper and read Chapter IV of the manuscript on shock waves.

Monday, June 16.

Spent most of the morning getting in touch with the people I want to see, making travel arrangements, etc.

Just before lunch went to the L. A. Municipal Airport to North American Aircraft Inc. to visit the Aerophysics Laboratory. Met W. Bollay, but talked to him only very briefly as he had to leave. Dr. Randels took charge. I discussed their report on the water wave analogy for supersonic flow of compressible gases and asked questions about some points which puzzled me, but did not get very satisfactory answers. The people who use the water wave analogy seemingly do not care to investigate the fundamental basis for that theory---with some justification since they are not primarily interested in water waves but in gas dynamics. I then saw the actual experiments consisting in dragging an object through shallow water and observing the wave patterns. The angle for oblique shocks does not come out right unless the model is long enough, i.e. unless the "wave lengths" are sufficiently long compared with the depth. Had lunch with

Dr. W. C. Randels - in charge of investigating the water wave analogy.

Dr. A. Vanszonyi - pupil of Emmons

Dr. J. J. Gilvarry - physicist from Princeton

J. R. Bruman - makes the experiments on water waves.

At lunch we spent most of the time discussing Friedrichs's recent result that shocks cannot begin in the interior of a region in steady 2-dimensional supersonic flow. Vaszonyi was very sceptical, perhaps because I could not explain it too well.

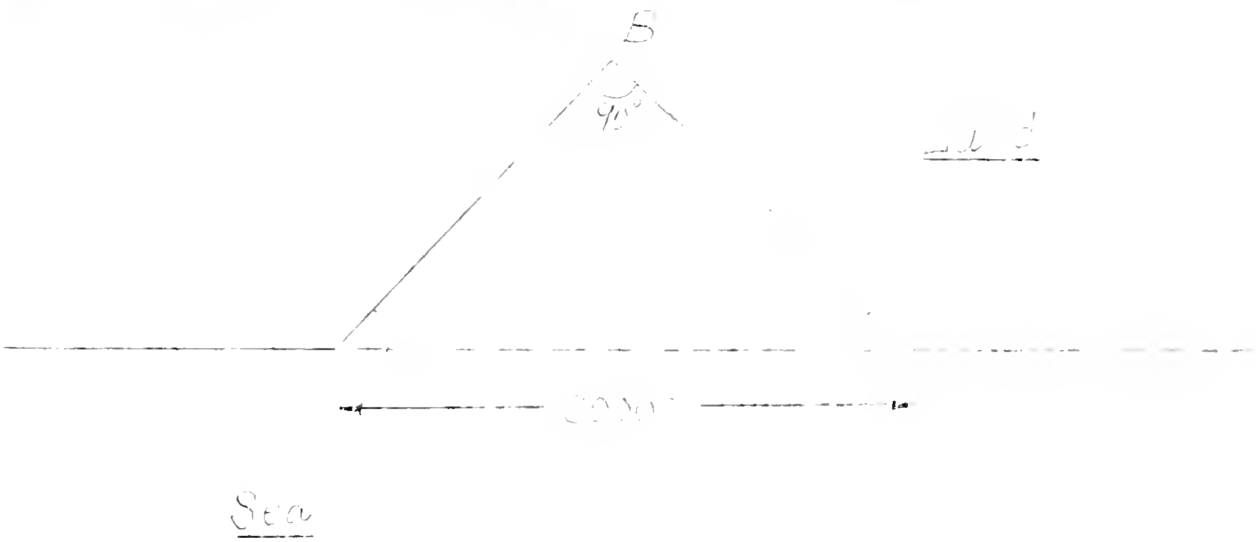
Tuesday, June 17.

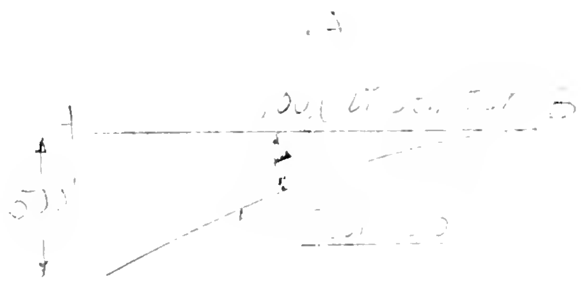
Up early and left L. A. for Del Mar, where I was met at the station and taken to the Scripps Institute. I met Dr. Walter Munk for the first time. W. M. is in charge of the research on water waves at the Scripps Institute. We discussed our work very privately for an hour or so until lunch, and decided that I should outline what our

group has been doing in the last year or so to a group of the people here in the afternoon. This was done. I talked for nearly two hours, with many interruptions for discussion, in the course of which we managed to clear up a number of points. The thing of principal interest to Munk and his people is the theory of breaking of waves, and they are very much interested in our approach, which differs from Munk's. Munk operates with the notion of the solitary wave, which I feel is wrong for reasons to be set down in a moment, while Munk correctly says that our method yields results not always in good accord with the observation on ocean beaches. A comparison of the two methods follows: Both methods work with nonlinear shallow water theories (these approximations to the exact hydrodynamic theory which are accurate for long waves in shallow water). However, I assume that the motion is not a steady motion and I am able to compute changes in shape of a wave moving into still water. The theory is exactly analogous to gas dynamics theory, and the formation of a breaker corresponds exactly to the formation of a shock in a tube. Munk reasons on the basis of a special steady motion furnished by the theory of the so-called solitary wave---a wave form which can propagate unchanged in form, at least in water of constant depth. The trouble is that this wave has a form which is symmetrical about the crest, and that seems to me to be rather wide of the mark for breakers in many cases---to my eye they seem to get very steep in front. (Our theory furnishes this behavior). On the other hand, Munk points out that the observations indicate that waves break as soon as the ratio of amplitude to depth exceeds a certain value---about 1.3---and that the solitary wave approach makes it possible to understand this at least in part, while my method indicates that the breaking phenomenon is practically independent of the amplitude-depth ratio. The fact is, however, that the two approaches are not so contradictory, for the following reasons. Both theories are approximations to the exact theory which arise essentially through developments with respect to the depth of the water. The theory used by Munk carries the development to terms of higher order than is the case for the theory I use, but assumes a steady motion and also one of very special character. Thus the theory I use

After lunch with Eckart we returned to Scripps and I talked with Dr. Sverdrup, the head of the Scripps Institution. Our talk ranged over a variety of topics, from water waves to general educational and research policies. I was surprised to learn that Sverdrup is returning to Norway as head of an Institute of some kind and will stay there permanently. He has been in this country eleven years. It is too bad to lose him.

After talking with Sverdrup I went to work once more with Munk and Arthur. They are at present much concerned with what are to be called tsunamis--a Japanese word for waves in the ocean arising from earthquakes--and are studying the observations on the most recent one which originated in the Aleutians and caused considerable damage in parts of Hawaii. They hope eventually to learn enough to be able to make predictions, perhaps from the seismic records. They want me to calculate the wave amplitudes in the following special case and to devise methods which they could use generally in similar cases. The case in question is that of a bay at Hilo (I think) which has roughly the following dimensions:





At the entrance to the bay a steady wave with a twenty minute period is to be assumed (and wave length corresponding to the depth there). The bottom is to be taken as cylindrical, with a parabola as generating curve and horizontal generators. There is a real difficulty, except in knowing the appropriate boundary condition to use along the shore line. One might assume perfect reflection, perfect absorption, or partial reflection and absorption. It was decided to assume perfect reflection, so that the solution will be a standing wave.

At about 5 P. M. Arthur and I went swimming in the surf. The water was cold, but not as cold as it usually is at Jones Beach, Long Island, N. Y., and the waves were coming in quite high and with long straight crests. It was a pleasant change to swim in water waves rather than to talk about them. Although I have been out here four times, this was the first time I had swum in the Pacific. After swimming, Arthur took me to his home for dinner, and I spent a very pleasant evening with him and his wife. Arthur would like to study with our group, perhaps a year from now, and I encouraged him to do it. He is the type of person we can help.

Thursday, June 19.

Most of the morning I spent at the Scripps Institute working with Arthur and trying to help him with some purely mathematical problems concerned with obtaining solutions for non-steady waves in water of finite depth in the vicinity of the wave front. Arthur of course had the same kind of troubles as Friedman had in calculating ship waves in water of finite depth, i.e. that the stationary phase approximation is not accurate, and the power series representation converges too slowly, at the region in question. Friedman's results on these questions should be published.

One of my main objects in going to the Pacific coast was to try to get a collection of photographic material on water waves under a wide variety of conditions as illustrative material for the book I am writing on water waves. [illegible] were very generous and gave me copies of any photographs they had—and they have many excellent ones.

[illegible] than I [illegible] train back to Los Angeles, arriving [illegible] out [illegible]. I went to Pasadena [illegible] the [illegible] of Cal. Tech, who [illegible] former [illegible] members of [illegible], [illegible], suggested that I might stay.

Friday, June 2[illegible].

I talked to [illegible] for [illegible] on [illegible] of our work on water waves in general, and [illegible] breaking of waves in particular. [illegible] this work that refers to the breaking of dams, [illegible] of flood waves in rivers [illegible] generally. It seems that there is even some practical interest in the subject of roll waves, which I had thought was more a theoretical curiosity, than anything else. [illegible] that surges of this character [illegible] occur in [illegible] channels in the Los Angeles region in the rainy season and that the sides of the channels must be made high enough to contain the maximum surges. It may be that a part of the work we have in view for the future will throw some light on the subject of roll waves, which seems very mysterious at present.

[illegible] introduced [illegible] to Dr. [illegible] [illegible]arner, [illegible] works in experimental hydraulics. In particular, he seems to be in charge of the large and elaborate model of Guam harbor (it is ~100 ft. in diameter) which they are studying with a view to planning the breakwater designs in the inner harbor to protect the anchorages, dry docks, etc. from the effects of the wave motion transmitted from the ocean into the harbor through the harbor entrance. The thing is done very elaborately. [illegible]arner gave me a considerable number of the photographs they have of the waves inside and outside the harbor, as obtained from the model. Some of these will be excellent for my book. I have strongly the impression that one could perhaps do a good bit of theoretical work on harbor design which might lead to methods of calculation which would in turn make a lot of extensive model tests unnecessary.

In the afternoon I was introduced to Dr. [illegible] Liepmann and his wife by [illegible]. Liepmann was very much interested in [illegible]'s recent result proving that shocks cannot develop in the interior of a fluid. Liepmann will probably visit us in New York in the course of the summer. I paid a visit to Bohnenblust. He talked mostly about non-linear vibrations.

H. S. Tsien (now at M. I. T., but formerly at Cal. Tech.) is in Pasadena on his way to China where he is going for a visit of a few months. He talked for an hour in the afternoon, and I went to [illegible] later for dinner that evening. Tsien, [illegible], Lagerstrom, and I had a very good dinner at a French restaurant in Pasadena and spent a pleasant evening together.

Saturday, June 21.

I took my first day off since I left New York and went to Long Beach for swimming with [illegible] and a number of his friends. The swimming was good---though not as good as it [illegible] because the surf was not high enough---and the weather was hot. In fact, I got my face pretty badly sunburned.

Sunday, June 22.

I flew from Los Angeles to San Francisco, arriving at noon. [illegible] (both of whom [illegible] with our group at [illegible]) [illegible], but visiting with his family [illegible] met me at the airport. We went to a restaurant not far from Palo Alto for lunch, then went up to a place called [illegible] where there is a beautiful grove of sequoias. [illegible] We arrived in Berkeley at about 10:00 P. M. I was exhausted and went to bed immediately.

Monday, June 23.

Practically the entire day was spent with the people in the faculties at the University of California at Berkeley. Most of the day I was with [illegible] Johnson, Associate Professor of Mechanical Engineering, who works chiefly in the field of [illegible]

they

linear theories if one wants to obtain a progressive wave rather than a standing wave. Johnson had in the main the opposite view to that of Arthur and Munk, i.e. he felt (as I am inclined to feel, too) that most of the incoming energy is absorbed, i.e. is lost, mainly at the shore line through the breaking of the wave. Johnson took me to talk with a young man named John D. Isaacs who is one of the so-called "eyes - ears - and - nose" men who had been in Hawaii to collect information about the recent tsunami there. He told me that the indications were that breaking occurred almost everywhere along the coast for both the incoming wave and that most of the damage was caused by the first, or possibly the second, wave. In other words, the practically interesting case may well be that in which the unsteady initial motion is treated, rather than a periodic steady state; and, in addition, it may be closer to the facts to assume absorption of all incoming wave energy rather than perfect reflection. More correspondence on this point with the people at La Jolla would seem to be in order.

At La Jolla and Cal. Tech. I asked for, and got, many excellent photographs of water waves from Johnson. I must get busy and get our book on water waves together.

I contacted Dr. S. Schaaf (until recently a member of our group) and talked to him about his work in the Engineering Division at Berkeley. He is working on a project concerned with flow of highly rarefied gases at supersonic speeds, and is very enthusiastic about the work. [illegible] had spoken of the possibility that Schaaf might help Johnson and his people on the theoretical side for work on flow in open channels and Schaaf promised me that he would do so.

Schaaf took me to visit G. C. Evans, head of the mathematics department, whom I have known for some time. We talked mostly gossip and news about mathematicians and mutual acquaintances and also a little bit about some interesting problems in potential theory in three dimensions which have come up in some of our recent work on water waves.

One of the main objects of my [illegible] [illegible] about many problems with [illegible], [illegible] still [illegible] us on various [illegible] problems. [illegible], [illegible] re-cently (after many years of bachelorhood) [illegible] in [illegible] mathematics, since he was [illegible] planning a year's trip around the world with his bride.

<u>Tuesday, June 24 to [illegible]</u>

Took a [illegible] [illegible] Owens, who will [illegible] our group [illegible]. [illegible] first through Placerville to [illegible], then over [illegible] to Nevada and followed the [illegible] of the [illegible] to Mono Lake. Then [illegible] up the [illegible] Pass (elevation over [illegible] feet) [illegible] the Valley. Climbed [illegible] Peak (10,6[illegible] feet) and swam in Lake Elizabeth ([illegible] feet), which was really cold---it is fed by [illegible] of snow on the mountain [illegible] it. Then [illegible] the Valley [illegible]. This is a sight that [illegible] descriptions. From there to [illegible], where there are hundreds of [illegible] ---[illegible] I drove [illegible] photographs. [illegible] to be seen to be [illegible]. [illegible] Friday afternoon--- a long [illegible] drive. I had been in California three times before the present trip, but I had never seen [illegible] mountains before. They are wonderful, but not as [illegible] are on the whole not quite as [illegible]. Moreover, [illegible] of Nevada desert on one hand and [illegible] on the other is something quite wonderful which [illegible] of course [illegible] the Alps.

<u>Saturday [illegible] Sunday, June [illegible]</u>

I went to Palo Alto to visit [illegible] at Stanford. [illegible] with [illegible] Schiffer [illegible] group, who [illegible] at Stanford this summer. I also talked with [illegible] project devoted to [illegible]. On Saturday evening [illegible] visited [illegible] from [illegible] student [illegible] Munich. [illegible]

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appeared. This one deals with relaxation oscillations, and we must get a copy of it.

Just before lunch I looked up J. K. Vennard, a former colleague from the College of Engineering of N. Y. U., who is now at Stanford Vennard works in hydraulics. It was too bad that I had so little time to talk with him since it seems that he also is taking an interest in the subject of waves in open channels and wants to try to develop experimental techniques for studying non-steady motions in this field of problems.

At 3:30 P. M. I left San Francisco by plane for Los Angeles, where I took a plane for New York which left at 6:30 P. M. and arrived in New York at 7:45 P. M. (both the local standard time). The plane was one of the new D. C.6's and was very steady.
I arrived home tired but contented in every way with the results of my trip. Things worked out better for me this time than on any other of the long trips I have taken.

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